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ART. I. — 1. *Medicine in Modern Times, or Discourses delivered at a Meeting of the British Medical Association at Oxford.* London: Macmillan & Co. 1869. 12mo.

(1.) *The General Relations of Medicine in Modern Times.* By HENRY W. ACLAND, M. D., F. R. S., LL. D., etc., Regius Professor of Medicine in the University of Oxford, President of the Association.

(2.) *Clinical Observations in Relation to Medicine in Modern Times.* By SIR W. W. GULL, Bart., M. D., D. C. L. Oxon.

(3.) *Therapeutical Research in Relation to Medicine in Modern Times, as illustrated by Researches into the Action of Mercury on the Biliary Secretion.* Report by PROFESSOR J. HUGHES BENNETT, M. D., F. R. S. E., etc.

2. *The Physiology and Clinical Uses of the Sphygmograph.* By F. A. MAHOMED, Student of Guy's Hospital. London Medical Times and Gazette, 1872, Jan. 20, Feb. 3, Feb. 24, March 2, March 23, April 13, May 18, etc., etc.

3. *On the Use of the Ophthalmoscope in Diseases of the Nervous System and of the Kidneys; also in certain other General Diseases.* By THOMAS CLIFFORD ALLBUTT, M. A., M. D., Cantab., etc. London and New York: Macmillan & Co. 1871. 8vo.

4. *On the Temperature in Disease*, from the 2d German Edition [*Das Verhalten der Eigenwärme in Krankheiten.* Leipzig. 1870]. By C. A. WUNDERLICH. Translated by W. B.

WOODMAN. New Sydenham Society's Publications, Vol. XLIX. London. 1871. 8vo.

5. *De l'Électrization Localisée et de son Application à la Pathologie et à la Therapeutique par Courants induits et par Courants galvaniques interrompus et continus.* Par le DOCTEUR DUCHENNE (de Boulogne). Troisième édition entièrement refondue, etc., etc. Paris. 1872. 8vo.
6. *On the Pathology and Treatment of Cholera.* By GEORGE JOHNSON, F. R. C. P., Physician to King's Hospital. Medico-Chirurgical Transactions. Vol. L. Art. X. London. 1867.

THE long array of titles in medical literature we have thought proper to associate with this article need not excite in the reader any apprehension that he will be called upon to assist in the process of conventional criticism. But we have selected them for the reason that, each and all, they march upon the highest planes of medical thought, and are themselves the very standard-bearers of its advance. Upon their authority we shall rely for the ideas we hope to develop, and it is with their assistance that we propose to sketch in consecutive narration such accepted general principles and such special but essential technicalities as shall constitute in reality an essay upon the thesis of Modern Medicine.

Medical men are charged with a liability to fall into one or the other of two opposite errors. They are charged with either being too content to regard their profession exclusively as an art, and with rather exulting in the assumption of an attitude of pure empiricism, or, on the other hand, there is ascribed to them an ambition to associate medicine with the dazzling but often delusive propositions asserted from time to time with a confidence which rarely accompanies even the best demonstrated facts in science. The truth is, we cannot establish with exactness or logical precision many of the fundamental notions to which medicine, as a profession, owes its very existence. Often we have to depend upon personal conviction alone, reasoning to ourselves from facts which we cannot explain, and acting in emergencies solely upon our rapid estimates of the probable. Pure Science admits no uncertain elements, but we cannot wait for her elimination of them; and when a phy-

sician, upon a balance of probabilities alone, acts, as he must, with a promptness flush with his decision, he is only like the navigator who trusts to his instincts in the tempest as readily as to his observations in the calm.

The older physicians conducted their observations in the true spirit of science, and reasoned with a caution which grew out of their consciousness of the uncertainty of the ground to be explored, and of the intrinsic inadequacy of their methods. Within certain limits we can place far more reliance upon the results of our own observations; but with more extended and precise methods comes the possibility of being found wanting in their characteristics of patience and care; while the ingenious mechanical appliances upon which we depend may be as much the measure of weakness as the proof of strength; and though by their aid the immediate judgment is rendered less difficult, the art of judging as a process of reasoning may eventually come to languish from disuse.

Without any sympathy with purely empirical medicine, we are still less in accord with that disposition to remote theorization which assumes that the spirit of modern medicine requires us to subordinate its advance to the vast and exhaustive generalizations of the science of the day. We can fit to no place in practical medicine the new *dicta* of the speculative physicists which assert that "an action of which we are immediately cognizant is but the result of the operation of the solar heat upon and through independent and correlative existences; that all things in this system are capable only of interchange; that there can be no destruction of that which exists, and no creation of new energy"; "that the human mind itself, emotion, will, and all their phenomena were once latent in a fiery cloud"; "that all our poetry and science and art, Plato, Shakespeare, Newton, are potential in the fires of the sun."

Now, instead of demanding upon what rests this projection of human thought into the infinite unknown, some of our outposts concede the very key to our position by the surrender of all notion of a vital principle or a Final Cause. This theorem of the conservation of energy controlling the laws of affinity and bringing all vital phenomena within the domain of physical necessity enters into every conception of organic

change; and as a corollary, if we cannot separate medicine from the indirect any more than from the direct influence of science, we must be governed by such remote physical speculations, and, abandoning all previous inductions, substitute for them a mere collection of undemonstrable postulates.

We pretend to have learned the methods by which the earth's crust became fitted to produce the chemical conditions which render the evolutions of organisms possible, but we cannot describe the commonest chemical changes going on every moment of our lives in our own bodies. We assume to fix the periods of the earliest geological transformations, but we cannot define the simplest of the vital processes. In the words of the chemist Bertholet, "We know nothing of any one of them thoroughly, since a perfect knowledge of any one involves the perfect knowledge of all the laws and all the forces which combine to produce it; in other words, a perfect knowledge of the universe."

The difficulties with which medicine has to deal are not due to the vastness of its included subjects, nor to any doubt as to their basis in science, but to the fact that it is itself a science which is complicated with an art and operating upon one of the obscurest of chemical processes. Nothing can be true in medicine that is not based upon unalterable law, and the law that underlies all therapeutic effort rests upon the maxim that living matter must act definitely in definite conditions. When failures are explained (says Mr. Mill, we believe), they become part of the law to which they were supposed to be an exception. Chances in disease are its variable causes, and there can be no assumption of erratic procedure, only to account for deviations for which no better reason can be given.

While the advance in medicine is in a great measure, but not so absolutely as some of our authors imagine, correlative with the general advance in science, it is by no means so certain that those departments with which medicine is the most closely connected are marching with the rapidity and precision of others with which we can claim no immediate relations. In the discussions of vital theories, which surely is where general science impinges most undeniably upon our pre-emptive domain, we find little else than opposed and controverted theories, with

mere fancies more or less whimsical, and dogmas in every stage of assertion and rebuttal. Chemistry, on the contrary, till quite recently exclusively analytical, has just entered upon a new career of combinations; and as there are really no limits to the possibilities of synthesis, we can follow it no further than the threshold, and must henceforth gaze upon its progress from afar, content with what our pharmacist shall distil from it, and grateful for what the chemical physiologist shall give us (and take back again) from year to year. But apart from all lofty connections and in its own career, medicine, while stationary with all learning for many centuries, is now fairly abreast of its tributary sciences, while as a practical art it lays under contribution the highest developments of all other arts.

As the physician can never more hope to comprehend more than an insignificant portion of any one of the allied sciences, it is consolatory to feel that in limiting the conception of professional duty and requirements to the single department of clinical medicine, the pure medical scientist of the future will be a far superior intellectual resultant to the quasi-universal sciolist of the past. For, as Sir William Gull says, "if it were possible to conjoin in one human intelligence all that is now known of all other sciences, such knowledge would be compatible with entire ignorance of clinical medicine." And bringing the subject into these narrow limits which we intend to claim as the only proper sphere of the medicine of the day, as the phenomena of living tissues are not explained by their chemical composition, so the phenomena of disease are not explained by the knowledge of healthy tissues or by the action of healthy organs. There are problems to which we are not bound to give a scientific solution, but which involve an action within the resources of art. We find ourselves surrounded by ailments and suffering, and in the main we know how to relieve them. The outcome of our knowledge is not philosophical contemplation, but it *is* adaptation and action: we cannot wait for absolute methods, nor shun the contingency of failure. And then we apply our *art*, "leaving physiological questions to the physiologist, and chemical questions to the chemist."

Having thus reduced our conception of medicine to its clinical phases alone, in dissent from the more ambitious flights of

Professor Acland, and having shown, we trust, that while elucidated by, it is not comprehended in the kindred branches of learning, we propose, before commencing the special subjects included in our general purpose, to point out a few changes in the broader ways of medical thinking.

In uniform conditions of human life, individuals are mainly alike, and are equally susceptible to physical ailments of whatever origin. Their occupations are the same, their food at given seasons is the same. Among savage nations, therefore, as the type of an identity of condition, we are not surprised to find whole tribes or communities carried off by a general epidemic, or by a single disease, like leprosy, plague, or small-pox. From the diversity of conditions in civilized life, on the contrary, there arises an immense variety of ailments to be studied. The diagnosis of these ailments, or the art of distinguishing a given one from all others, rests upon the basis of knowing the sum of the possible in morbid processes. But what is exhaustive in the pathology of to-day may be very far behindhand to-morrow. For while the whole subject of morbid anatomy is said to have been studied out to its ultimate, to be in fact an exhausted mine, such a conception of it must only turn, we fancy, upon whether its phenomena are now to be ranged as causes or as resultants. And next to the possible in disease come the second elements in the probable, as learned from the largest and most varied experience.

Our danger will always lie in deviating from our own narrow but trustworthy path, and wandering away into the tempting fields of physical or chemical analogy. Yielding to external importunity, we are induced at one time to look upon the human body as a sort of undeveloped galvanic battery, and at another we fancy it to be simply an animated oxidizer, and that all our ailments arise from defects in its machinery or obstructions in its chemical processes. Not very long ago the microscope seemed likely to disintegrate all large conception of clinical phenomena, and to tie us down to what Nelaton called the beetle-eyed habit of observation. Nor should we leave out of the reckoning a method of studying disease by columns and averages, calling itself in its short-lived day the "Numerical System," and which never, as it pretended to do, at any time replaced

opinions by facts, but which really retarded the scientific induction and philosophical study which are the characteristics and the pride of the present day. Its labors were wasted in mere counting-house puerilities with disjointed and irrelevant details, and for years some of the foremost men in France and America, but never, we believe, in Germany or England, found no better occupation for their faculties than the inconsequent tabulations and the fatuous arithmetic of this happily effete substitute for medical observation.

Modern medicine inclines to regard diseases no longer as distinct entities, but rather as perverted life-processes with their proper access, culmination, and decline, requiring only time and rest for their completion; and it is not by setting up distinct conceptions in opposition to the standards of health that these processes are best to be studied. Perverted functions must be attributed to failure of action, not to excess of it, and we are to watch the very first deviations and shortcomings from the condition of health itself. Organic strength is now known to exist in full nutrition and in healthy function only, never in their perversions. But we are still hampered by the old nomenclature, and with the clearest conceptions we cannot always avoid confusion and apparent contradictions. The terms "sthenic" and "asthenic" are relics of the old "phlogiston" which itself still survives in the expression "anti-phlogistic." *Strength* and *weakness* are descriptions of no value in semeiology; they are deduced from subjective and only superficial phenomena. The quickened pulse, the acute delirium, the burning fever, all once supposed to indicate a preternatural strength, are but the tokens of a deficient balance-power, and heat itself has come to be the surest measure of organic waste.

The terms "zymosis" and "zymotic disease" have for a long time held a position in medicine for which no valid reason can be given. There is really no clinical condition which we can intelligibly or with any propriety define as *fermentation.* The specific tokens of a fever are to be found in its attendant tissue-changes, not in the multiplication of hypothetical organisms. "Zymosis" in classification is the superfluous survivor of the old scholasticism which generally explained the obscure by something that was still more obscure.

In every part of the body are developed proclivities to some special morbid action, with its appropriate mode of repair, whose course of life is as natural as that of the tissues from which it springs. We may take as the type the plastic lymph which helps on the resolution of the inflammations of the lungs, of the heart, and of their investing membranes. Knowing these intrinsic tendencies, we trace back the morbid action to its sources, and are prepared for its coming results. The brain also, without apparent cause, is liable in the adult to the formation of tumors, and in the young to insidious softening; from certain external phenomena we are led to suspect these conditions, finding a meaning in elements once entirely overlooked; and mere symptoms have come to be of little value in any diseases of the brain. Studying them only from books, we read of the most diverse affections presenting identical symptoms, and the most extensive lesions developing no symptoms at all; while in their pathological study the whole catalogue of symptoms may have one after the other appeared, and not the trace of disease have been left.

Certain textures are prone to morbid action, while others are almost sure to be free from it. Portions of a single continuous tissue have the same liability or exemption, and why, we cannot say; but knowing the fact, we may distinguish with certainty the structures of the parts affected. We recognize analogies between the infantile and the senile affections, as different and as like as "the tints of the rising and of the setting sun." Special forms of convulsions, of abdominal pains, of skin diseases, of uric-acid deposits, are common to both periods, and reveal alike the failing power of degenerating tissues in age, and inadequacy of power in the growing tissues of infancy.

A number of very common acute disorders are now believed to be secondary to latent chronic lesions, or dependent upon distinct degenerations, the potential lesion of causation being unnoticed in the storm of symptoms it has excited. Some of the acute inflammations within the abdomen and the encephalon are only the ultimates of a morbid process of many years' duration, and a disease, long regarded as local, turns out to be not the sole or immediate cause of death, or even the most important change. And we now comprehend, as never before, the old

Hippocratic axiom, "Diseases do not fall upon men instantaneously, but being collected by slow degrees explode with accumulated force." These unquestioned facts of the changed relations of acute and chronic disease, like all reversals of long-established opinions, are in danger of leading to the opposite extreme, and Sir William Gull especially seems to entertain the conviction that acute disease is, upon the whole, more likely to be the result and not the cause of accompanying chronic lesion. Where extreme views of this sort are once fixed, the pathology can always be made to conform to the theory; and as it is very difficult in *post-mortem* inspections to determine with exactness the cause of death, we should be wary lest the scalpel and the microscope give us back only our own *a priori* conceptions. The general belief that chronic conditions are mainly the result of irremediable acute ones is too firmly fixed to be reversed but with the greatest caution.*

* After these passages were prepared for the press, we received from England a copy of the "Medical Press," published in Dublin, of the issue of January 15. Among its contents are the reports of the medical attendants of the late Emperor Napoleon III. upon the causes of his death. It will be remembered, perhaps, that, from its suddenness, this was at first attributed to *embolus* and afterwards to *chloroform*. We shall transcribe the lesions actually found after death. They furnish a striking comment upon the determination with which even a philosophical mind will carry out a preconceived theory, and, as we intimated in the text, make the pathological appearances conform to it. They furnish, in fact, a swift and complete confirmation of the objections we ventured to raise in our criticism of Sir William Gull's opinion as to the relations between acute and chronic disease. That very eminent man could not have desired a better opportunity to illustrate his theory, and it was inevitable that he should avail himself of it. We feel bound, therefore, to reproduce the facts in the case at length; as we had already asserted that any fixed general notion of this kind would be likely to lead to error under circumstances where the fact of priority of condition must be a matter of conjecture mainly.

The autopsy of the Emperor was made by Dr. J. Burdon-Sanderson, in the presence of those whose names are appended to his report. Sir William Gull does not appear to have been present, but he has placed upon record his dissent from the opinions of those who were, upon a single point only, and that point involves the very matter upon which we had taken issue with his theory.

It would seem that an impression prevailed in England that the true nature of the Emperor's complaint had never been detected by his French advisers; and the facts which were developed in the communications this belief elicited prove the long existence of the more potential cause of death, and disprove entirely the assumption upon which Sir William founds his non-concurrence; while they also show, which is not so much to our purpose, however, that the French faculty were perfectly acquainted with the nature of the case.

There are drawbacks in the medicine of the period. It is inclined to neglect the broader study of general diseases for the closer methods in diagnosis alone, and to assure it by ingenious aids. Its exclusive study of small portions of the body limits the mind to a localized pathology. It is even to the disadvantage of skilled auscultation that it diverts attention from the more general cause which has induced the local affection; and, finally, it is only the local or special diseases that are now considered fit subjects for monographs.

On the whole, the more careful study of morbid processes has resulted in the isolation, not only of the individual, but of the separate life of his special organs and tissues; of which the natural and fortunate consequence is a method of treatment ap-

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"The most important result of the examination was, that the kidneys were found to be involved in the inflammatory effects produced by the irritation of the vesical calculus (which must have been in the bladder several years) to a degree which was not suspected; and, if it had been supposed, could not have been ascertained. Death took place by failure of the circulation, and was attributed to the general constitutional state of the patient. The disease of the kidneys was of such a nature and so far advanced, that it would in any case have shortly determined a fatal result.

"Signed by all present:—

"J. Burdon-Sanderson, M. D.
Dr. Conneau.
" Le Baron Corvisart.
Henry Thompson.
J. T. Clover.
John Foster.

"Camden Place, Chislehurst, January 10, 1873, 6.30 p. m."

Sir William Gull records a separate opinion on one point only, namely, the origin of the calculus, in the following terms:—

"I desire to express the opinion that the phosphate of lime calculus which formed the nucleus of the mass was the *result* of the prior cystitis, not the cause of it. This nucleus was of uncertain duration, and may even have been more recent than supposed in the appended report." "It seems, to my judgment, more in accordance with clinical experience to regard the cystitis [inflammation of the bladder] as the *prior* lesion, and that this by extension, as is common in such cases, affected subsequently the ureters and pelvis of the kidneys. No doubt in the later stages of the malady the calculus by its formation and increase was an augmenting cause of the lesions." (The italics are ours.)

Now, as we learn from the *Union Médicale*, "On July 1, 1870, the Emperor Napoleon feeling very ill, a consultation took place between Doctors Nelaton, Ricord, Fauvel, Corvisart, and See. Their diagnosis was that of stone, and an immediate operation was recommended." But from *political considerations* "the disease was allowed to go on augmenting for two years and a half." As the "Press" remarks, "the report is in strict accordance with clinical experience, and, what is more to the point, with the *clinical history of the case itself.*"

plied to the individual, and not to the name we may have to give to his complaint. His own life constitutes the individual, and all the alterations of that life, therefore, are individual and not general. One person dies in an epidemic, and another recovers, from causes peculiar to these individuals. Only to the charlatan is disease a congeries of symptoms, for every one of which its special drug has been catalogued.

Certain abnormal conditions have now ceased to be regarded as diseases. Mere "ailing" is as natural a process in advancing age as health. "The Diseases of Advanced Life," so ably treated by Dr. Machlachlan, who studied them among the Greenwich pensioners in his charge, might more properly, we fancy, be entitled "The Natural History of Old Age." The natural changes of age only simulate the diseases of a younger period, and even well-marked special diseases once fastened irremediably upon the system lose their distinctive characteristics and become merged in general decay; and this view has been recently corroborated most strikingly by the facts developed in relation to a recent discovery at Vienna.

The now well-known tendency of renal disease and its consequent ill-nutrition to induce chronic or sub-acute inflammations in the encephalon, we can allude to more conveniently in another section of this article.

A great variety of related phenomena from a single cause, varied by individual tendencies, were formerly classed as half a dozen different diseases. The term epilepsy once denoted a special morbid state with convulsions and unconsciousness. We see in it now a condition of disturbed molecular nerve force, such as is likely to occur in any other aggregate of molecules, in which may occur not only the common phenomena of epilepsy, but coma without convulsions, paralysis following convulsions, sudden and transient mania, and peculiar forms of neuralgia; we must therefore cease to give reality to such an affection apart from its phenomena.

Among the new connections in which old symptoms appear, there are two which deserve special mention as trophies of more minute exactness in study, namely, embolus and locomotor ataxia. To Virchow of Berlin we are indebted for a pathological discovery which has cleared up many obscurities.

Minute doses of fibrine, or coagula of blood, called *thrombi*, become detached from the chambers or valves of the heart, travelling either in small particles or in masses to the smaller arteries, or they may course along the veins back to the heart. To the coagulation of blood in the living vessels, starting from the fact that arrested blood coagulates at the temperature of the body, 98° F., Virchow has developed his theory of the "inflammations of veins" as it was before regarded, and explains many results formerly attributed to "palsy of the heart," "retrocession of gout," "metastasis," "sudden death," etc. To the formation of clot Virchow gave the name of *thrombosis*. The projection onwards of the clot or thrombus along the vascular system is called embolism, and the whole series of actions is now known specifically as embolus. From this discovery, as it may almost be called, many things before involved in mystery are brought to light and arrange themselves in definite order, enabling us to anticipate the occurrence of other results, and to prevent them.

The *ataxie locomotrice*, first described by Dr. Duchenne of Boulogne, is the result of a degeneration of the posterior columns of the cord which, after all the discussion, is the main and distinctive seat. Dr. Baillie, however, had proximated the matter in his description of a sort of "paraplegia, from excessive morbid secretion of the cerebro-spinal fluid, which in the erect posture gravitates to the lower part of the sheath" and pressing upon the cord embarrasses the co-ordination of movements. The isolation of the motor columns in this affection harmonizes with an old physiological notion, that these parts had their own vitality and a separate function, being mere commissures or connecting bands: textures limited to such a function may have a lower vitality than others whose functions are more essential, and at the same time a greater tendency to disease than those of higher vitality.

Having stated much that has been learned of late, let us confess to a little ignorance and to a few wants. We do *not* know why it is that in the two poisons, cholera and diphtheria, both developed on mucous surfaces from the blood, all the salines and water are poured out in the one, and a fatal effusion of fibrine takes place in the other. The cattle plague and

the horse disease are as far removed as possible beyond the reach of cognition or prevention. Do what we will, epidemics must always be anticipated in some form or other, and hygiene can never replace therapeutics. Finally, we are sadly in want of simple and ready means to recognize chemically the urinary morbid products as easily as we now detect sugar and albumen. They are largely connected with changes in health and disease, and chemistry still owes such methods to medicine.

The Sphygmograph. — Our purpose requires no comment upon the pulse. Well studied by the ancients, during the Middle Ages any reliance upon it was generally regarded as a superstition in the category of astrology; "it was doubted if its varieties, as described by Galen, could really be observed or understood." The pulse, however, will always be recognized as the unerring index of a function whose continuance is essential to life. Not only must the heart not cease its action for a single moment, but we are suspicious of the slightest token that it wearies or faints at its work, for the issues of its halting are the issues of life and death. Apart from its immediate connection with the special disorders of this great centre, the pulse has not lost its position as chief among the methods of finding out the working of every portion of our physical machinery. The sense of touch, however, is not only deceptive, but it cannot be taught; no teacher can ever impart to his pupil the experience or knowledge that his own well-educated finger derives from the pulse. The sense of sight, therefore, is not only a much more reliable guide in itself, but it is a far better medium for description and instruction.

Galileo was the first to conceive the idea of recording accurately and visibly the characters of the pulse, and he constructed for the purpose what he called a *pulsilogia*, which made no impression in its day and left no trace but its name. Vierordt invented a sphygmograph, but it was full of fallacies and of no clinical value. The name of Marey is now exclusively associated with the modern instrument which is made to register the pulse-shock and the wave of the blood column along the arteries.

The sphygmograph is essentially an arrangement of levers

by which the wave as transmitted is increased in apparent size, or rather exaggerated in action, by movements similar to those of a small upon a larger wheel. An ivory pad rests upon the artery, usually that at the wrist, the rush of blood along the vessel lifting the pad perceptibly even to the eye. But however slight the movement, it is sufficiently exaggerated by the levers to procure a distinct tracing of its normal outline and of its every variation. The impulse received by the ivory is transmitted by a long slip of light and flexible wood, at the opposite or free extremity of which is affixed a fine steel point, which we will call the *stylus*. By a secondary arrangement, a slide, placed at a right angle with the lever and in contact with the stylus, is made to move by mechanical appliances from right to left across its point, so as to give when in motion, to the liftings of the pad and to the movements of the lever and of the stylus, the effect of tracings in free running hand from left to right. The rate at which the slide moves is made to correspond with the time required by each pulse-wave to effect distinctly the whole of its individual tracing, that is, the rate of about four inches in ten seconds. At this rate of speed, each impulse occupies about four tenths of an inch in space upon the slide, and we here premise that the spaces and lengths to be spoken of assume a healthy pulse, without pressure and with the motion of the slide graduated to this rate. We may also state that the instrument as it was left by Marey, like most mechanical appliances requiring practice and skill in manipulation as well as judgment in induction, has usually sunk out of sight as fast as introduced. It had faults and imperfections which speedily destroyed its reputation for reliability, and at last it came to be scarcely mentioned even in medical textbooks. In fact, it had dropped into what seemed to be its natural place as simply a physiological curiosity. The monographs upon its uses, especially that by Dr. Burdon-Sanderson, are more in the nature of scientific treatises than adequate guides to its clinical use.

These considerations were probably what induced Mr. Mahomed, a student of Guy's Hospital, to commence an extended and exclusive study of the subject, and by his improvements in the instrument, accommodating its movements to the feeblest

pulse, and with the advantages of a nearly perfect mechanism, he has been enabled to present, not only a new method, but a new literature of the sphygmograph.

Although a pen actually dipped in ink may be made to do sphygmographic writing upon white paper, the best register is a tracing by the steel point upon smoked paper made portable and permanent by varnish. And if the reader will now attempt to form a mental picture as he goes along with us, we will endeavor to clear up the mystery of the sphygmograph as intelligibly as it can be done without diagrams, at least to give some conception of the simple process by which the heart is made to write out a description of its own ailments at the end of a telegraphic wire a thousand miles away.

The first impulse imparted to the pad by the artery sends the stylus suddenly upwards on the smoked paper, scratching a straight line of about half an inch or less in vertical height. This first up-stroke is called the percussion stroke, and proceeds from the shock received by the blood-column as a solid body, on the bursting open of the aortic valves as the blood leaves the heart for the general circulation,—a shock communicated almost instantaneously to the whole arterial system. In the normal condition this stroke is distinct from the true pulse-wave; if confused or identical with it, it becomes a sign of disease. It is the index of a healthy suddenness and force of the heart's contraction, and measures its muscular strength or weakness in acute disease, but it is not a guide as to the relative amount of blood forced into the aorta for distribution. After marking the percussion stroke, the stylus next drops about a tenth of an inch, at a slight distance away from the vertical line and making a very acute angle with it, and its descent is cut short at this point, and sometimes pushed slightly up again, by the distension of the artery by the true tidal wave. Its next fall forms a curve with the concavity downwards. Up to this point the whole tracing is called the percussion wave, from the starting-point of the percussion stroke to the end of the first curve; and the space between the vertical line and the next regular ascent of the stylus measures the time elapsing between the opening of the aortic valves and the arrival at the wrist (of the tidal impulse). Next follows the

second up-stroke and fall of about half the height of the vertical line, whose force and comparative height measure the amount of blood forced into the arteries; and the degree of pressure, usually about twenty pounds, required to extinguish it, shows the actual propulsive force of the heart's contraction, a much slighter pressure being sufficient to efface the percussion wave. We are now about half-way over the inter-vertical space, as we may call it. The next motion traces a curve with its concavity upward, and the notch from which it starts is called the aortic notch, and the impulse ends with the closure of the aortic valves. The second elevation is due to arterial expansion, and the whole stroke ends with a gradual fall of the stylus to the base line corresponding with the collapse of the artery. This base line, drawn through the bottoms of all the vertical lines, Marey's *ligne d'ensemble*, is curved, with its convexity upward on forced expiration, and conversely, on forced inspiration, it forms a downward curve, and might be called the respiratory line. The whole pulse, then, is divided into two parts, corresponding with the contraction and the dilatation of the chambers of the heart; and we are now prepared to see what the deviations from these normal characteristics indicate, as derived from a few simple inductions.

First, the graduated amount of pressure required to efface the tidal wave shows a hard or a soft pulse. Secondly, the verticality of the percussion stroke and its proportion to the tidal wave show a slow and feeble or a sharp and strong heart action. Thirdly, the height of the tidal stroke shows the amount of blood expelled from the ventricles. Fourthly, the degree of convexity of the succeeding curve indicates the amount of obstruction to the arterial collapse from any cause. Fifthly, the dicrotic ascent or rebound indicates disturbed relations between the heart's contraction and the arterial tension. Sixthly, the spaces indicate the relative duration between contraction and dilatation; and seventhly, the convexities of the *lignes d'ensemble* show the effect of the act of respiration upon the pulse.

The pulse of every individual is as characteristic of him as his intonations or carriage, and we do not doubt but that the sphygmograph might sometimes settle questions of personal

identity; but in disease the pulse loses this individuality, and varies with the general circulation. The sphygmograph has differentiated between diseased conditions of the brain, which even the ophthalmoscope had left in doubt. But it should not be forgotten that its action is circumscribed and its value limited. It is the index of the vital and dynamical condition of the circulation alone. We have only to add that tracings can be taken from the apex-beat of the heart, when they are called cardiographs; they are wanting in the finer details we have above specified.

The Ophthalmoscope. — What we have to say of the ophthalmoscope will be limited to its uses in general diseases; with its employment by the specialists in the examination of local affections we have nothing to do. Nor is it necessary to describe the well-known little mirror which is a part of the programme in the visit to the "ophthalmic surgeon." Invented by Helmholtz, but brought into its present use and nearly perfected by Liebreich, it certainly has worked a revolution in ophthalmic science. It may not be superfluous, however, to remark, that the instrument in question is primarily a concave reflecting mirror with a perforation in its centre, which catches the rays from a light placed by the side of the patient's head, and throws them back to the bottom or *fundus* of the eye, which is itself a part of the magnifying power. Any transparent reflecting body is an ophthalmoscope, and with an ordinary lamp sufficient light is returned from the concavity of an ordinary watch-glass to exhibit all the details at the back of the eye; and it is said that it possesses the advantages of not requiring dilatation of the pupil by atropia. We do not believe, however, in simplifying or extemporizing processes in so important a matter as diagnosis of optic changes. The most perfect instruments and skilled manipulation, with every accessory to facilitate the examination, should be made use of, nor should our own judgment be trusted till after long practice and many comparisons.

The specialists Graefe and Liebreich were the first to observe the effect of constitutional lesions in the structure of the eye; and although great progress has been made in their study by

both French and English physicians, the first English classic on the subject is but hot from the press, — the beautiful volume by Dr. Allbutt. Ophthalmoscopy has been for years gaining ground in medicine entirely distinct from its specialistic utility. But a short time ago an eminent London physician said to the author, "It is nonsense to pretend to localize diseases within the encephalon; ingenious guesses you may make, but such guesses seldom prove to be worth much in the dead-house."

Clinical observation in this direction has been stimulated by the great advance in our knowledge of the morbid and the minute anatomy of the central nervous system. The brain is so complex and inaccessible an organ that it has kept out of the reach of the rigorous analysis which has of late changed the whole aspect of medicine. The inspection of its actual morbid processes will go far to disperse those transcendental habits of thought which, driven from every other position, still have their foothold in the brain, and to establish the fact that as its functions are only the movements and relations of its tissues, so the diseases of the encephalon can only be the abnormal action of its component parts.

Disturbed vision or absolute loss of it has always been known as an attendant upon diseases of the brain. Soon after Bright's discovery of the connection between albuminuria and renal disease, amaurosis was noticed as an occasional symptom of the new disease. The ophthalmoscope discloses not only the mode by which sight is thus affected, but that very process itself makes important revelations as to the remote cause residing in the kidneys. It not only tells what we could not otherwise know, but it is sometimes the only solution of a difficult problem, and in distinguishing and foretelling results its value is immediate and irreplaceable.

But it must not be forgotten that the use of the ophthalmoscope in general medicine is limited to that state of the intracranial circulation shown in the vascular system of the optic nerve and retina; nor is it infallible here. As we have intimated, the interpretation of phenomena by it is not easy, and the absence of change in the retina may coexist with abnormal cerebral circulation.

The indication of cerebral disease through the optic nerve is due to its great vascularity, its large share of connective tissue, and its contiguity with the parts at the base of the brain. The nerve and the retina are independent of each other in their vascular supply; the central artery and vein of the retina lie among the fibres of the nerve, but distribute no branches to it. But in the optic disk, which is the great centre of observation, a network of vessels connects together the nerve and retinal arteries, etc. The disk therefore is closely connected with the cerebral vascular system, and is the best index of its condition and disturbances. An inflamed or congested optic nerve may be watched from hour to hour, and false conclusions be avoided, even if the cause cannot always be known and located, and its nature, often more important than its seat, be ascertained. Before the connection between changes at the back of the eye and the effect of intra-cranial blood pressure upon vision were known, the utmost confusion prevailed as to the cause of the disturbance. At one time a tumor would be found pressing upon the optic nerve or ganglia; at another identical phenomena would occur without tumor, and the sympathetic system, the original sin of baffled nerve science, would be suggested as the offender. The most careful clinical observers are still in doubt as to the actual value of optic changes in the diagnosis of tumor, though Dr. Allbutt avers that nothing is more certain than that "intra-cranial tumors commonly give rise to changes in the optic disk." Without entering into any discussion of disputed points, we propose, in accordance with our plan, to state a few illustrations of the clinical uses of the ophthalmoscope.

The "choked disk," *ischæmia*, is due chiefly to such causes as distention of the ophthalmic veins, inflammation of the brain membranes, hydrocephalus (the affections of youth), and tumors; conversely, acute and chronic softening, hemorrhage, hardening and arterial degeneration (the affections of age), do *not* cause it. The appearance called neuro-retinitis is an extension of inflammation from the brain, especially if its seat be near the optic tracts. What seems remarkable is, that no inferences can be made as to the state of the optic disk from degrees of vision. Choked disk, nerve inflammation, and

advanced atrophy may all exist without any affection of sight, without pain, and without sensitiveness to light.

The study of some particular affection will give a better insight into the value of the instrument in general diseases than any other course, and we will take for this purpose epilepsy. Between the paroxysms of epilepsy, the optic disk is observed to be of a deeper red, the vessels are larger than normal; during the convulsions the disk is of a pinkish color, the arteries are small, the veins large; after the fit the disk is pale, the arteries small, and the veins are unchanged. As consciousness returns, the disk becomes of a deeper red, the arteries are larger, and the usual retinal appearances return. The stage of stupor indicates a general condition of anæmia. These appearances confirm the present theory that epilepsy is due to anæmia, and not to congestion of the cerebral vessels. If maniacal excitement follows the fit, there is even more congestion. The degree of congestion is in direct ratio to the number of fits and their severity. Now when the fits are reduced in number by bromide, the congestion is lessened; but if the fits recur with their former frequency, the congestion returns. It is inferred therefore that the reduction of congestion is not due to the immediate and direct action of the bromide on the blood-vessels, but is due to the absolute reduction in the number of the fits, possibly through some other action of the bromide, which loses its power to control the morbid tendency after the constant vascular disturbance caused by the fits has led to marked passive congestion, but frequently controls it when congestion is not so far advanced. We thus arrive at a modification of the general belief as to the action of bromide absolutely, and at some knowledge whether it is likely to be of benefit in a given case.

In renal disease the most conspicuous change is retinitis, with subsequent degenerations of and about the disk, so characteristic as to be at once recognized as due to albuminuria; not confined to any one form of renal disease, but most common with contracted granular kidney. It is believed that these initiative changes in the retina are set up originally by uræmic poisoning. The mirror reveals two classes of eye-affection, those due to uræmia, which are sudden and temporary, and more gradual impairments arising from visual degeneration.

Spinal disease, if chronic or if slow in access, is detected by optic changes. If it arise from injury, the higher its seat the sooner the after-changes appear. These changes are, first, simple primary atrophy, which is *never* the result of injury, but is common in chronic degeneration of the cord and in *locomotor ataxia*. Secondly, indistinguishable retinal arteries with swollen, dark, and tortuous veins; the disk red, its borders lost; these are seen only in injury, never in degeneration or ataxia.

As new instruments of precision open new avenues to knowledge, so an improved method stimulates research, and the mind itself is invigorated by the new opportunities, and from habits of accurate and industrious observation comes that intimate knowledge which is founded upon facts, however they may oppose tradition.

The Thermometer in Disease. — In 1754, Antonius de Haën, the first known teacher of clinical medicine in the Hospital of Vienna, taught his pupils to ascertain the temperature of the body in fevers according to the measurements of the thermometer, and not through the impressions received by the hand. It had not escaped notice, even at that period, that in the cold stage of ague, with chattering teeth and shivering skin, pallid and cold from contraction of its blood-vessels, the temperature of the body beneath was rapidly rising, and even with the rude instruments of the period the increase of heat could be accurately measured. The hand of the physician and the feelings of the patient are alike fallacious and unsatisfactory.

The instrument of precision now exclusively used in observations of animal temperature is a sensitive mercurial thermometer, graded between 85° and 115° Fahrenheit, self-registering, and measuring with fifths or even tenths of a degree. As a rule, the temperature is taken at the armpit, the skin being folded over so as entirely to cover the bulb, at different periods both of the day and night, and the daily changes are accurately noted on the proper thermometer charts. We must start from the normal standard of fluctuations within the limits of health, and with the correlations of animal heat with the pulse and respiration.

The normal heat of sheltered portions of the human body is 98°; and a rising above 99°.5, and a depression below 97°.3, if persistent, are certain signs that disease of some definite type is present, or, as we should express it, some special deviation from health. But there have long been on record observations on normal temperature under variable circumstances within the limits of health: those communicated by Sir John Davy to the Royal Society disclose the following facts: —

In temperate climates the *maximum* temperature is in the early morning; after waking it fluctuates till nightfall, and is lowest about midnight, but the average difference is less than one degree (0°.82). In tropical regions the *minimum* is in the early morning; after waking it fluctuates, and is highest during the day. Davy also observed that "an undue degree of elevation was some criterion of the intensity of diseased action."

Animal heat is affected by a great variety of collateral circumstances, but the great distinction between the variations of health and those of disease is, that the first are temporary and within narrow limits, amounting to mere fractions of a degree; those of disease are of much greater extent, and persist during the continuance of such disease. Besides changes due to physical exertion, atmospheric changes, etc., it is ascertained that sustained mental exertion depresses heat half a degree; that after a full meal or the use of alcohol, heat is at first reduced, as digestion advances the temperature rises; that the average temperature in the tropics is one degree higher than in temperate zones; that the temperature is more sensitive and more readily and rapidly affected than either the pulse or the respiration, especially in disease. The degree of abnormal increase of heat is usually in proportion to an increased frequency of the pulse (about one degree to ten pulsations), and is in direct ratio to the severity of other general signs of disease; and where disproportion or incongruity exists between the heat and the pulse, or other febrile phenomena, as sometimes happens, the accurate measurement of the temperature is the most to be relied upon. The maintenance of the temperature within its known normal limits is a complete assurance that any disturbance shown by other clinical indications

must be local and unimportant; and in military practice it discloses whether disease is feigned or real.

The conditions in which the thermometer serves a purpose which no other means can attain, are fever and tissue-waste. The essential element in fever is preternatural heat. Our knowledge of the natural history of febrile diseases has reached great exactitude and certainty during the past few years, but heat, after all, is the important feature. Whatever other theories have prevailed during twenty centuries, heat bears the same relation to fever as in the time of Galen. It is never absent; without it fever cannot exist. Rigors and chills are mere external phenomena; the sensations of heat and cold are subjective impressions due to the special condition of the peripheral nerves, not to any absolute condition of the temperature; and conversely, while the skin may be ice-cold to a bystander, the patient may feel that he is burning up within.

Some of the most exact and useful observations were made long ago in our own country by Dr. Joseph Jones, late Surgeon-General of the Confederate forces; but beyond everything in the way of recorded observations are those of Wunderlich, the Leipzig professor, whose book, translated by the New Sydenham Society, contains the record of half a million of exact thermometric observations, following the continuous process of nearly every human form of disease, and comparing the results from more than five thousand patients. Among them are instances where disease was demonstrated by the thermometer before it was possible to detect it through any other means, and sometimes a definite diagnosis was based upon a single observation. Sir John Davy gives a singular instance of rise in temperature where it was impossible to recognize disease by any of the ordinary methods. A lunatic soldier had for many weeks a temperature of 104°. The insane do not suffer from cold and heat like others, and certain organic lesions are apt to occur in them without the usual symptoms. This man made no complaint; his appetite and digestion were good. He died at the end of a month of acute tuberculosis, without any other symptom from first to last than this great and persistent elevation of temperature. There were ulcers in the larynx which

had not affected his voice; there were large cavities in the lungs which had not caused cough; there was ulceration in the intestines without diarrhœa; and various other lesions, all masked and latent.

There is no form of illness characterized by fever which the thermometer cannot accurately distinguish, and recognize the type of; and if it be of an anomalous type, its departure from known types may be defined and described. But its most important use is to mark the daily changes in the condition of the individual affected: the development and progress of his febrile state, its abatement, subsidence, and defervescence, and the approach of local lesions. The term "defervescence" indicates the period during which the temperature of the fevered body is declining from the intense degree of heat which marks the state of accession towards the normal standard; if sudden, the defervescence is called *crisis;* if gradual, *lysis.*

Some of the best established clinical inductions in medical thermometry are as follows: The highest temperatures just before death have been observed in scarlet fever and in lock-jaw. When the temperature goes above 99°.5 we know that the patient is ill from some definite cause; if it rises to from 101° to 105°, febrile phenomena are present with great severity; if it goes above 105°, the patient is in imminent danger; in a range from 106° to 109°, a fatal result is all but certain and is immediately at hand. In any disease, a temperature of 104° or 105° shows that its progress is not checked, and that complications are still possible. The thermometer, therefore, is the best and earliest warning of an additional development of disease, or that some visceral complication has occurred; and if in any accurately distinguished case of disease the temperature departs from its normal and typical range for that disease, we are to anticipate trouble. As convalescence only commences when the disease process ends, we know that it cannot occur till the normal temperature returns and maintains itself through all periods of the day and night, and during convalescence any considerable fall of the thermometer foreshadows collapse.

The morbid development of heat is sometimes associated with more, sometimes with less, abundant bodily excreta than

in health. The temperature and the amount of the excretions bear certain as yet undetermined relations to each other. The loss of weight in a patient is due to an increased and rapid elimination of material with increased tissue changes, and is associated with increase of temperature, — more certainly as regards urea than with the other excretions; whence it is concluded that heat is attended with increased elimination of urea, and therefore with greater tissue change. While the product of metamorphosis, as determined by the excreta, may be diminished in fever, there may still be increased metamorphosis with less elimination. Finally the pulse, the respiration, and the temperature, each and all, represent forces at work within the living body which can be measured with great exactness, and such measurements show how closely the expenditure of forces is related to the excreta which represent the waste of tissue in health and in disease; show, also, the decline in that nervous control that regulates the functions in health: that is, in the force that limits the working of all other forces.

Localized electricity, or electrization, as it is commonly called, is now conceded to be a most powerful agent in the treatment of disease; but, to borrow the language of Dr. Russell Reynolds, "it is useful, useless, or mischievous, according to the manner in which it is applied"; and although the greater part of what we have to say about it will be as a mode of treatment, it is introduced here by virtue of the rank it has just begun to take as a most profound and scientific method of diagnosis.

The term "electrization" is generic, and should never be otherwise employed. It is common in medicine to restrict electric effects to the three varieties, of friction, of contact, and of induction. Frictional electricity, which Faraday himself called the Franklinic, is also known as "static"; while the two other forms are confounded under the common name of "dynamic." A primitive form of the static was the now obsolete electric bath, or the immersion of the feet in buckets filled with electric eels; the only forms of its present application are through sparks, or by means of shocks from the Leyden jar, or by insulation upon the glass stool, when the patient is charged

like a prime conductor. Static electrization, however, may be regarded as practically in disuse, the milder forms having no appreciable tonic effect, and the great "tension" required from the Leyden jar to reach well-covered muscles or deep-seated parts cause such disturbances as render its application difficult, if not impracticable. Curative results, however, have been produced when other modes have failed, by its application to the larynx in obstinate nervous aphonia, than which, as some writer has said, "nothing short of being hanged could be more unpleasant."

Dynamic electricity is employed nearly equally under the two forms of galvanism and Faradisation, and is that variety which is developed by chemical decomposition. Galvanism, or the "interrupted continuous current," is of low intensity in its action on the nerves and muscles; but it produces chemical or "electrolytic" results, as in the coagulation of blood, the mortification of tissues, and a calorific action, which are not to be obtained from the Franklinic or even from the Faradic processes.

Faradism, or the "induced," "magneto-electric," "voltao-magnetic," or "voltao-dynamic" electricity, as it is variously called, is of very high tension, and more like the Franklinic than the galvanic. Its chemical and calorific action are insignificant; it has not the burning and heat of galvanism, but produces marked contraction of the muscles and powerful action on the nerves, both of sensation and of motion. It is induced, but only of momentary duration; and these momentary currents, made by breaking and resuming the galvanic current, are effected so rapidly that a great many of only momentary duration, but of very high tension, pass in both directions in a single second of time.

The question which Dr. Duchenne has undertaken to solve is: Are the physical and therapeutical qualities of the different forms of electrization identical, and can they be indifferently applied? From the time of Remak, whose name is identified with electrization in Germany, very acrimonious discussion has been going on upon this point, and it has never been more bitter than since the late Franco-German war, cropping out in quite a number of French and German prefaces we have

had occasion to look at. The *dictum* of the Regent of the University of Berlin, "that Germany was a contented nation, and coveted nothing beyond its own boundaries," rankles deeply within the bosom of the Gaul. But while in Germany the school of Remak still adheres to galvanism, it is still in France that, in Becquerel and others, Dr. Duchenne finds his most persistent opponents. In England and America both currents are employed: in general practice the Faradic, with specialists the galvanic, more or less exclusively. Dr. Duchenne maintains that the two currents, whose physical action is so dissimilar, ought to produce different therapeutic actions, and each should correspond to special indications.

Electropathy is an art so difficult that slight shades of difference should not be allowed to complicate treatment, and the question mainly is whether any such difference is one of kind or of degree. Practically, it is of degree only; the object is to get electricity in the form most suitable for the emergency. In electro-surgery both currents will avail to disperse tumors, heal ulcers, or hasten absorption. The galvanic is said to be superior to the Faradic in power of overcoming resistance, and it might therefore be expected to act more effectually on the brain, the spinal cord, and the sympathetic nerve, — the anatomical position of these parts requiring considerable resistance to be overcome. It also produces muscular contractions where the Faradic fails; and after a certain amount of treatment by the galvanic current, paralyzed muscles frequently resume their susceptibility to the Faradic. Thirdly, it has a different and more chemical effect for the purposes of cautery, using elements that generate large quantities of electricity, and combining them in a particular way. An ordinary single element, or a large number arranged for intensity, are but feebly electrolytic, and the alleged superiority of galvanism in neuralgia, atrophy, or rheumatism is probably due to the molecular or other tissue changes produced by this chemical action.

On the other hand, Faradism, through its frequent interruptions, produces muscular contractions if applied anywhere over the muscle. While in galvanism one of the electrodes must be placed upon the motor nerve of the muscle, and the

galvanic current requires to be broken to produce effects, the Faradic is already in the condition of constant interruption. But the advantages of Faradization are most marked in general electrization, its powerful tonic effects being largely due to the passive exercise and consequent important tissue changes resulting from the myriads of muscular contractions, while it is less likely to produce unpleasant or injurious effects. Under particular circumstances it is the only one that can be used with benefit or safety.

Dr. Duchenne claims that Faradization is the only therapeutic agent which, limited to the skin, can produce most acute sensations, which cease at once with the operation, and can be graduated from the slightest tickling to the most intense pain, passing through intermediate degrees or going suddenly from one extreme to the other without ever disorganizing the skin or leaving any trace upon its surface beyond a slight erythema or elevation. Such an agent must respond to innumerable indications, from restoring simply lost sensibility to the skin itself to the most powerful revulsion in deep-seated neuralgias. It can be tempered, moreover, to the degree of excitability of any individual, and even of each region of the body; and in the intense current required for certain muscular affections, induction is the only appreciable form. Claiming this, Dr. Duchenne admits that its electrolytic action is too feeble to coagulate the blood in aneurism or to affect the atrophization of morbid growths.

We have considered electrization, so far, in its relations to treatment, and have but little space left to devote to any account of its uses in diagnosis. Its conclusions in this field of study, however, are not universally accepted; in fact, are not very widely known. Professor Acland says, "The exploration of the nervous system by electrical agencies in evidence of chemical alterations and in proof of corresponding changes in the organism (by Dr. Duchenne of Boulogne) shows the certain advance of physical inquiry as applied to disease." It is due to the great reputation, moreover, of Dr. Duchenne to state that he was the first to describe the character and pathology of the progressive paralyses known respectively as, (1.) Progressive muscular atrophy; (2.) Progressive locomotor ataxia;

(3.) Glosso-labio-laryngeal paralysis: all of which types are now well recognized in practice. And in this his third edition, which we have just examined fresh from the press, he adds another morbid condition not hitherto described, but not less fatal in its march and termination, namely, pseudo-hyper-trophic paralysis or myo-sclerosis; also a form of infantile or "obstetrical" paralysis, and an acute and a sub-acute *anterior* spinal paralysis of the adult. These therapeutical researches are the result and aggregate of Dr. Duchenne's indefatigable clinical and pathological studies, most of them having been communicated during the past few years to the different scientific societies of France.

The report on the *action of mercury* by Dr. Hughes Bennett and the committee appointed by the British Medical Association must have greatly astonished the average British practitioner. Although a few medical men now profess to have long been sceptical as to the action of mercury, nothing was ever more generally believed than that it stimulated the biliary secretion. Abernethy's eternal blue-pill is a matter of history, and it was almost the only medicine that Sir Astley Cooper knew anything about. Sir Benjamin Brodie was asked why he always gave blue-pill in certain cases, and could afford to answer, "because he always *had* given it, and it had always been right."

The point for the committee to determine was whether mercury was a chologogue, that is, a bile-stimulant, and these were not the first experiments that had been undertaken to determine the specific action of mercury. Professor Nasse, in 1852, seemed to have ascertained by direct experiment that calomel did increase the absolute quantity of bile, but that it diminished the amount of the solid contents. Holliker and Müller demonstrated that the first dose of mercury increased the quantity of bile, and that subsequent doses diminished it in quantity but increased its consistency. Dr. Mosler investigated in order to determine "what substances introduced into the blood were found afterwards in the bile," and his conclusions were that calomel, in large or in small doses, "does not pass so readily into the bile nor produce the marked increase of the biliary secretion that medical men imagine." Dr. Scott found "a dim-

inution in the amount of bile and bile solids secreted after the administration of large doses of calomel." This was the state of the question in literature.

Dr. Bennett tells us that his committee comprised "the skill of the anatomist, the analytical power of the chemist, and the varied knowledge, theoretical and practical, of the histologist, physiologist, physicist, pathologist, and therapeutist, as well as the physician whose knowledge of diagnosis is unimpeachable." Every instrument, appliance, and chemical, together with a public hospital and the sum of one hundred and twenty-five dollars (!), were placed at the disposal of the committee. At their first meeting, November 16, 1866, the first and, as we think, fundamentally vicious conclusion was arrived at, that no kind of examination of the *dejecta* could yield trustworthy results; supposing that the characteristic constituents of the bile found their way into the alvine evacuations unchanged, "the imperfections in the analytical method (!) render their quantitative analysis impossible," either by Hoppeseyler's method or by the simpler process with alcoholic extracts. (Hoppeseyler, professor at Tübingen, estimated the amount of bile acids from the effect which their solution exhibited upon the ray of polarized light, which Professsor Tyndall's experiments have made comprehensible.) We are to infer, therefore, that neither the bile acids nor the coloring matter were determined or estimated. For an examination of the excreta, they substituted the formation of biliary fistulæ in living animals, and the collection of the amount directly through such fistulæ from the gall-bladder.

The committee admit that, under the action of purgatives, unchanged bile is occasionally discharged from the system; and it is also evident to us, from an inspection of their numerous tables, that the biliary secretion was increased after the first dose of mercury. We observe also that the animal began to *fail* soon after the process of drawing off its bile commenced; that in fact the very organs and tissues experimented upon were on the road to death; that it was not only an animal which was not man, but a wounded and a diseased animal.

From the experiments, however, such as they were, and Dr. Bennett states that they were carefully repeated under every varying circumstance that could be thought of, the following

conclusions were reached: first, that in whatever form or dose, whether continuous moderate doses of blue-pill, minute and frequent doses of calomel, or large doses of it, mercury utterly fails to stimulate the liver. The constitutional action of mercury excited both slowly and rapidly by corrosive sublimate produced the same results. In poisonous doses the bichloride produced a marked diminution in the flow of bile. Mercurial inunction was followed by negative results; and it was regarded as proved that, so far from increasing, mercury, by its general depressing action on the system, actually diminished the amount of bile.

Now we submit that, as depression does lessen the vigor of the secretive process, a much more potential cause for depression is to be found in the condition of the animal from his wounds and the presence of the canula, and from the actual wasting disease engendered by the deprivation of bile, than lies in the giving of a few doses of calomel, which at first, as we have seen, before the constitutional symptoms began, actually increased the flow; and the next conclusions of the report corroborate this idea, for it was found, *secondly*, that abstinence from or considerable diminution of food checked the secretion of bile; and, thirdly, in deprivation of bile, emaciation comes on, the appetite fails, the excretions assume a peculiarly fœtid odor, and death occurs from inanition. Starvation diminishes the secretion, and nothing beyond giving food or supporting health increases it.

Whenever we hear of a *new anæsthetic*, we think of the time when the taking away of pain was to "harden society and rob God of the deep, earnest cries which arise in time of trouble for help," as the Scotch parsons told Sir James Simpson; and when even surgeons argued that "pain was a premonitory condition, no doubt fitting parts the subject of lesion to reparatory action," and that they would therefore "feel averse to its prevention"; when a famous living physician concluded a paper read before a London medical society, "Upon the Injurious Effects of Ether Inhalation," with queries as to "the desirability of removing pain"; when a great American surgeon declared, after some untoward details in giving ether, "that he would never again remain in a room where it was to be given."

After much experimentation for ether substitutes, a substance has been found which will actually manufacture chloroform in the blood, and, what is better, its invention is the result of purely *a priori* considerations. On the second day of June, 1869, Dr. Otto Liebreich, of the Pathological Institute of Berlin, introduced to the Medical Society of that capital what he defined as "a new hypnotic and anæsthetic agent"; this was the hydrate of chloral. Chloral itself was discovered some thirty years ago by Liebig as the product of anhydrous alcohol acted upon by dry chlorine gas, ultimating in a dense oily liquid, colorless, boiling at 210° with a sharp, pungent odor. Mixed with water this becomes the hydrate of chloral, and is converted into a white solid substance. The new agent became at once widely known, and in London Dr. B. W. Richardson, who is constantly experimenting with anæsthetics, was appointed to investigate it and report upon it.

The theory of its action, according to Liebreich, is that the hydrate of chloral, treated with an alkali, is resolved into chloroform, and into a formate which soon disappears; the blood is an alkaline fluid, therefore the hydrate, when it is absorbed, enters the blood, where its particles consume the surrounding alkali, and decomposition is effected after the required amount of alkali has been furnished by the blood. A minimum quantity of chloroform is then instantaneously formed, is set free in the tissues and passes to its first stage of action, the ganglia cells of the brain; as the chloroform increases in amount, its action extends to the spinal ganglia cells, and lastly to those of the heart corresponding with the slowest possible administration of chloroform, and passes out of the system as chloroform. From Liebreich's array of facts also was derived the necessary deduction that the agency at work was chloroform, chemically formed in the blood. There are few now who have not experienced or observed the profound and tranquil sleep produced by chloral; but it is the rival of opium rather than of ether, although it can be administered by inhalation. Besides producing sleep, it removes sensibility, reduces the animal temperature, causes extreme muscular relaxation, practically confirming the theory of its action as chloroform. Its physical action being clearly understood, its applications must be ex-

tremely varied, and in fact it is now employed to relieve almost every known abnormal symptom or condition of body or mind. Dr. Richardson's report concludes with a warm tribute to the "genius and industry of the distinguished Professor of Berlin who has placed in our hands so admirable and scientific a remedy."

With regard to *Cholera*, the novelty is in the treatment alone; its nature is universally understood. A poison is absorbed into and infects the blood, spoiling certain of its constituents, which are ejected through the mucous membrane of the alimentary canal. A primary blood disease is thus produced, undergoing enormous multiplication in the body; and the direct results of the alteration are changes in the function of respiration, while the copious discharges are the efforts of nature to throw off the noxious material, as a necessary process to recovery. From no other cause than a virulent blood poison does a person in full health become in a few minutes, and utterly without warning, shrivelled up, his whole body icy-cold, his face and hands turned purple, while death comes on in a few hours. We cannot refrain from reproducing the quotation very happily brought in by Dr. Aitkin: —

> "Its effect
> Holds such an enmity with blood of man
> That, swift as quicksilver, it courses through
> The natural gates and alleys of the body;
> And, with a sudden vigor, it doth posset
> And curd, like eager droppings into milk,
> The thin and wholesome blood."

The well-known and especial danger in cholera is also its proper and distinctive symptom, characterized by the French pathologists by the term "algide," the sensation and fact of *cold* being one of the most remarkable and constant phenomena; the thermometer in the axilla falling to 92°, more than six degrees below the natural standard in health, while under the tongue it is known to have fallen to 79°. Vomiting, purging, and cramps have long ceased to be regarded as essential phenomena. Hale and hearty men have been suddenly seized, and have collapsed, and died within five minutes, without any evacuation; and death has repeatedly occurred within two or

three hours after access, without any other cause than stagnation of the blood.

Dr. George Johnson, long known to the profession in connection with the correct pathology of kidney diseases, communicated to the Medico-Chirurgical Society of London, in 1868, the doctrine that the poison of cholera was received into the system from without, by inhalation or in food and water, and absorbed into the circulation; that if the vascular excretion was checked, the risk of fatal collapse was greatly increased; and that the object in treatment should be, not to *excite* discharges from the mucous surface, but to facilitate the removal of matters lodged there, and that elimination of the poison from the system was the condition of recovery.

Half a century ago the English physicians in India "aimed to get rid of the offensive morbid secretions" in cholera, but it was quite natural to attribute collapse to the drain of fluid matters from the blood by the profuse and repeated fluxes from the bowels and stomach; and the treatment this view suggested was the "locking-up" plan by opium and astringents and by stimulants. Dr. Johnson's opinions were not well received, and violent controversy grew out of them. It was soon settled that the collapse was not due to the evacuations, but that these were, in fact, eliminative of the poison. To establish relations of cause and effect between them, it should happen that the more profuse the discharge the more decided and certain should be the collapse; while in fact the most hopeless cases were those of scanty evacuations or of none at all. Also the continuance of the discharge should cause a more prolonged, deeper, and more dangerous collapse. But, on the contrary, patients not only emerge from the collapse while the evacuations are going on, but a cessation of them is always a fatal sign. If a vein is opened in cholera, after a moment or so, the dark treacly blood grows brighter, and the patient has been known to cry out that "it had made a new man of him"; therefore collapse has nothing in common with ordinary syncope, which is relieved by stimulants, but would be disastrously affected by blood-letting. Dr. Johnson's own paper in the "Transactions" is immediately followed by another, containing details of treatment upon his plan, on a large scale, in which the re-

sults corroborated the theory, and which demonstrated the remarkable fact that recovery never occurred without a continuance of the discharges or their restoration, if previously arrested.

So great a change in medical opinion was effected by the new views, that Sir Thomas Watson, the president of the society, upon a careful revision, has fully recanted his own published opinions and adopted those of Dr. Johnson in the last edition of his well-known work on the Principles and Practice of Physic, and he pays their author a noble tribute by declaring that "he has by his methodical display of facts, by his close and conclusive reasoning, now triumphantly established his own views of the pathology and treatment of cholera."

Lörstofer's Corpuscles. — During the past few years much attention has been given to the connection between what are called microscopic growths and contagious and infectious diseases. In Professor Stricker's laboratory in Vienna, especially, attempts have been making to explain different diseases, particularly the infectious ones, by the presence of fungous growths in the blood, and in the secretions and excretions, as well as in the tissues of the body, but with hitherto only negative results. In the blood of syphilitic patients especially it was hoped to find traces of a low grade of organism. Could such once be detected, the discovery would not only have a vast specialistic importance, but would give a positive basis to an assumed theory. Dr. Lörstofer conceived that the negative results were due to low magnifying powers, and to the fact that the experiments had all been made with fresh blood in which such bodies may have existed in a state too minute for detection, and that they might *grow* to be visible (this very idea of growth, we may remark in passing, being corollary to the very idea underlying the research), if the specimens could be preserved for examination without destroying the organisms. Lörstofer commenced his experiments in August, 1871; and in January, 1872, at a meeting of the Vienna Medical Society, he announced the discovery of a form of corpuscle peculiar to the blood of syphilitic patients, and through which the diagnosis of syphilis might be made by a microscopic examination of the blood.

In specimens properly prepared, in from one to three days, he averred that certain minute bright corpuscles became visi-

ble, some immovable, others undulating; in a few days more they grew to the size of true blood corpuscles, of irregular shape, with one or more projections. In a few days longer a vacuola was found in the larger corpuscles, which soon extended over the whole body, and terminated the development of the growth, nor could their shrivelling and further retrograde development be arrested; their relations of causation were not asserted.

While Professors Hebra, Skoda, and Stricker indorsed Lörstofer's statement, and affirmed that they had tested it with almost uniform success, others who had long worked without success in the same direction were not satisfied. Wedl pronounced the bodies described to be nothing but fat cells, and declared that they could be seen a few hours after the preparation was made if water were added to the blood; but as Lörstofer's corpuscles do not acquire a visible size till the fourth day, and disappear at once if water is added, it was argued that Wedl *could* not have seen them at all, as he had violated Lörstofer's conditions. Professor Stricker here took up the controversy, and Lörstofer himself seems to have been left out of it. Letters from Vienna to the English and American journals stated that the war speedily became outrageously personal and offensive.

Stricker himself forms no opinion as to their true nature, but pronounces them to be formations hitherto unknown in the blood. Meantime a syphilitic patient in whose blood they had been uncommonly numerous began to show signs of phthisis, which drew attention to another point, namely, the possibility of a general ill-nutrition being concerned in their production, and in fact they were at once found in cases of cancer, of tuberculosis, of Bright's disease, and of anæmia after small-pox. The investigation therefore seems to be sufficiently advanced to pronounce the new organisms not exclusively pathognomonic of syphilis, but as existing in the blood in certain cases of defective nutrition from long-lasting chronic diseases, and more particularly from syphilis. As yet there is no warrant for denying their possible existence in the blood of persons suffering from acute diseases, or even in the blood of comparatively healthy subjects.

We have now completed our task upon a plan kept always in

view; commencing with the general and larger relations of medicine, and showing where it would be better to reduce it in study to its clinical phases alone. We have selected the special subjects which best illustrate the latest or the highest methods in diagnosis, in therapeutics, and in pathology, and which were most likely to lend a uniform coloring to the picture. The medicine of to-day aspires to nothing more, and is content with nothing less, than to be the sum and the harmonization of all that can be gathered from observation and all that can be added by induction. It has sometimes been difficult in the sketch to avoid the appearance of marshalling before the reader a parade of imposing generalities; it was not always easy to escape from tiring him with burdensome details. And the success may lag behind the aspiration. Be that as it may, enough if only he, to whom nothing that is human is foreign, shall any more clearly discern the spirit that controls the science of modern medicine, or any more wisely distinguish the forces that direct its art.

W. O. Johnson, M. D.

Art. II. — Arthur Schopenhauer and his Pessimistic Philosophy.

We often hear people complain of the abstruseness of philosophy, and it may be no fault of theirs if philosophy remains unintelligible to them. But then, there are many other abstruse things, — celestial mechanics, for instance. Yet those who could not understand a single line of Laplace's work never think of denying the truth of mathematics, or the usefulness of astronomical induction, or the legitimacy even of astronomical speculation. This shows at once that the popular dread of philosophy cannot be due to what philosophy has in common with mathematics, namely, an abstruse terminology. Such a superficial difficulty would be as easily overcome in the one case as it is in the other, and the many attempts at popularizing philosophy would have been more successful than they have been if this were the only difficulty. Moreover, there is hardly any science